MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Dr. LEE, Vice-President, in the Chair.

Samuel Player, Esq., Duxford, Cambridgeshire, was balloted for and duly elected a Fellow of the Society.

On the Transit of Venus, 1882, December 6.

By the Astronomer Royal.

The *Monthly Notice* for 1857, May 8, contains a communication made by me to the Society on the means available for correcting the received measure of the Sun's distance from the Earth. Among the different subjects of observation applicable within some years to this object, I have particularly indicated the Transit of *Venus*, 1882, December 6, treated by the method of comparison of durations of transits at different places, as one which merits most careful consideration.

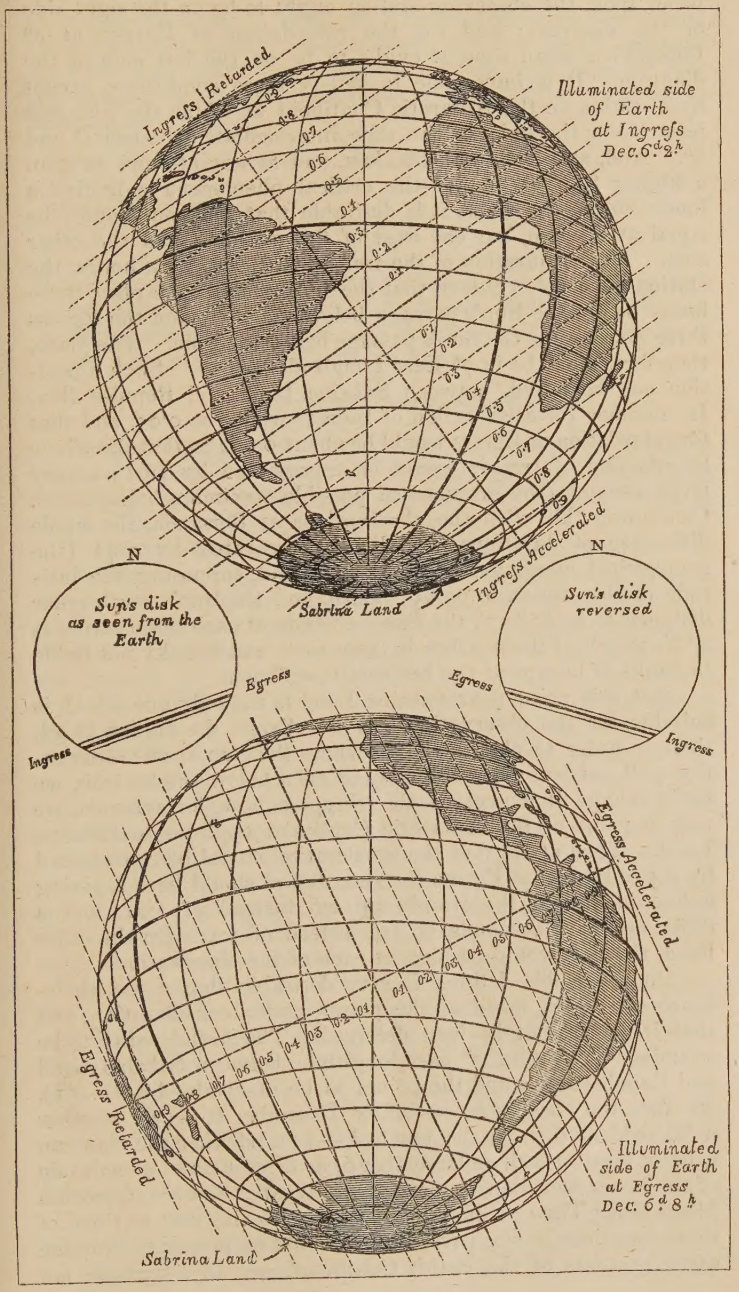
The small Maps of the Illuminated Sides of the Earth at the time of ingress and egress of *Venus* (accompanying my paper) were drawn on a projection which exhibited with sufficient clearness the countries where the transit would be visible, but which did not give facilities for estimating numerically the effect, at each locality, of the relative parallax of *Venus* and the Sun. A moment's consideration will show

that, if at each of the critical phenomena (Ingress and Egress) a plane be drawn cutting the centre of the Earth and touching that part of the Sun's limb at which the phenomenon happens, then the amount of acceleration or retardation of the time of the phenomenon, at any special locality, as attributable to parallax, depends merely on the magnitude of the perpendicular from that locality to the plane just mentioned. If an orthographic projection of the Earth's illuminated face be drawn, the curve upon the Earth's surface, which passes through all the points at which that perpendicular has a determinate value, is represented in that projection as a straight line, parallel to the line which represents the first-mentioned cutting plane. And, therefore, if we wish to indicate those points of the Earth at which the parallactic effect is $\frac{1}{10}$th or $\frac{2}{10}$ths of the maximum, we shall only have to draw, on such a projection, lines parallel to the central line which itself is parallel to the limb at which the phenomenon takes place, at distances from the central line equal to $\frac{1}{10}$ and $\frac{2}{10}$ of the Earth's radius.

At my request, Mr. Carpenter has prepared the accompanying Orthographic Projections of the Illuminated Sides of the Earth for Ingress and Egress, and has placed on them the lines marking the localities at which the effect of parallax will be $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, &c. of the maximum effect in both directions. A small circle for the Sun's disk and lines for the apparent path of *Venus* are also shown; and likewise the same circle and lines reversed (right to left), because in that state they represent the points of Ingress and Egress, as viewed in the same direction in which the illuminated side of the Earth is viewed, and therefore enable us at once to fix on the points of the Earth's limb where the parallactic effect is maximum.

Selecting, then, first the parts of the Earth at which the duration of transit would be shortest, it is seen at once that, in the seaboard of the United States of America, the Ingress is retarded by a quantity represented by 0·95, and the Egress is accelerated by a quantity which, on the mean, is 0·83 nearly; so that the whole shortening is represented by 1·78 (the geometrical possible maximum being 2·00). That locality, therefore, is very favourable. The Southern States, as far as the Gulf of Mexico, without much diminution of parallactic effect, would have the advantage of a higher Sun at Egress. Another favourable place is Bermuda; the parallactic number for Ingress is 0·9, and that for Egress is also 0·9; so that the whole shortening is 1·80. The circumstances, therefore, are exceedingly favourable for the selection of observing-stations at which the duration of transit will be much shortened.

Selecting, secondly, the parts of the Earth at which the duration of transit would be longest, it will be found that the choice is more limited, and the practical difficulties rather greater. For the acceleration of Ingress, at 2^h Greenwich


Ingress Retarded
Illuminated side of Earth at Ingress Dec. 6d 2h
Ingress Accelerated
N
Sun's disk as seen from the Earth
Sabrina Land
N
Sun's disk reversed
Egress
Egress
Ingress
Ingress
Egress Accelerated
Egress Retarded
Illuminated side of Earth at Egress Dec. 6d 8h
Sabrina Land

mean time, the observing-station ought to be on the right side of the diagram; and for the retardation of Egress, at 8^h Greenwich mean time, it ought to be on the left side of the diagram. It is impossible to satisfy these conditions, except by a station on the Antarctic Continent at which the Sun will pass below the Pole in the time intermediate between 2^h and 8^h. And, as the Sun will be low, and as any attempt to gain a higher Sun at one of the critical phenomena will give a lower Sun at the other, it is desirable that the hour-angles be equal at the two, but one west of the meridian and the other east. As the duration of the transit is (nearly) six hours, the station must be so chosen that the Ingress will take place three hours before the Sun's passage below the Pole, and the Egress three hours after the Sun's passage below the Pole. The place, therefore, must be in 7^h East Longitude nearly. Such a position can be found, between Sabrina Land and Repulse Bay. Its number for acceleration of Ingress would be 0·95, and that for retardation of Egress would be about 0·66; so that the whole lengthening of transit would be represented by 1·61; a very large amount (the geometrical possible maximum being 2·00). Combining this with the observations at Bermuda, the whole difference of durations would be represented by 3·41 (the geometrical maximum being 4·00). And, supposing the latitude of the station to be 65° South, and the Sun's south polar distance to be 67° 27′, the Sun's altitude at each observation is 4° 6′; which, if the weather is favourable and the sky not liable to banks of haze near the horizon, is sufficient.

But this point near Sabrina Land is the only one which is suitable for the observation. If we change the station at all, then, in order to obtain a place where the Sun at one observation will not be excessively low, or even below the horizon, we must change it a great deal. Suppose, now, for example, we took a point of the Antarctic Continent opposite to Sabrina Land. Then at Ingress the acceleration would be represented by 0·5 only, and at Egress the retardation would be 0·1, giving a lengthening of the transit by 0·6 instead of 1·61, and a difference with Bermuda of 2·4 instead of 3·4. On the other hand, the Sun would be high at each of the observations.

On the whole, I think it very desirable that a reconnaissance should be made of the points under consideration, and that it should not be long deferred. The first locality to be examined is that in 7^h east longitude, between Sabrina Land and Repulse Bay; and the points to be ascertained are,—(1), whether the coast is accessible on December 6,—(2), whether a latitude of 65° can be reached,—(3). whether the Sun can usually be seen well on December 6, at three hours on each side of the lower meridian, or more precisely, at 2^h and 8^h Greenwich Mean Solar Time. Should the answer to the first or third of these questions be negative, then it would be proper to examine other portions of the South Continent, say in longitude not

very different from 5^h west, but with no particular restriction, except that of gaining the highest possible south latitude. And the only point for inquiry would be, how well the Sun can usually be seen on December 6 at 2^h and 8^h Greenwich Mean Solar Time.

Royal Observatory, Greenwich, 1864, *May* 5.

Remarks upon the Statements of Messrs. Stone and Carpenter relating to Sir John Herschel's Figure of the Sinus Magnus in the Nebula of Orion. By Prof. G. P. Bond, Director of the Observatory of Harvard College.

(*Communicated by Mr. Carrington.*)

In the late Report of the Council of the Astronomical Society (*Monthly Notices*, vol. xxiv. pp. 92, 93) occurs the following passage relating to observations recently made at Greenwich upon the Nebula of *Orion*:—

"On the evening of January 11th the Nebula of *Orion* was splendidly seen. It was the opinion of the observers, Messrs. Stone and Carpenter, that the portions about the so-called jaws were not so accurately represented in the late drawing by Professor G. P. Bond as in that of Sir J. Herschel, printed in the *Results of the Cape Observations*, 1847. There is a squareness of outline about the so-called jaws, and an almost total absence of luminosity within them, in the drawing of Professor Bond, that certainly was not presented by the Nebula on this evening. The drawing of Sir J. Herschel appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws; the only marked deviation being that the wisp of luminous matter, 10^s following and 300 divisions north in Sir John Herschel's drawing, appeared to point more towards the trapezium than there represented. The other parts of the Nebula have not yet been examined with the same care as the portions about the jaws."

In offering a few words of comment upon these statements, I would remark at the outset that the drawing, for which I am responsible, is a proof taken from an *unfinished plate* of the Nebula about a year since. I have not, however, any desire to take advantage of this fact to screen the engraving from any censure which it may fairly deserve. Whoever attempts the impossible task of representing such an object faithfully upon paper will be certain to expose himself to the charge of inaccuracy in many important particulars, and just criticism is the surest means of advancing our knowledge of the subject; besides I am confident that the figure, as it stands, is capable in the main of a good defence. Its principal defect, in the

parts referred to, is much more in the relative intensity of the lights than in the drawing.

For example, Messrs. Stone and Carpenter find that "there is a squareness of outline about the so-called jaws, and an almost total absence of luminosity within them, in the drawing of Professor Bond, that certainly was not presented by the Nebula on this evening."

I find, on careful examination of a proof taken in the same stage of the plate, that barely *one-sixth* of the area within the "*fauces*" is represented as destitute of light, and that the proportion of surface illuminated in my drawing, instead of being less, is three times greater than in Sir J. Herschel's. I do not know of any figure of the Nebula which contains so small a proportion of absolutely dark ground as my own. The error into which Messrs. Stone and Carpenter have fallen I can explain only by supposing that they did not examine the engraving by daylight, or as closely by lamplight as they should have done. The fainter gradations of light are not there by accident, but are the results of a careful scrutiny of this region. The only areas quite destitute of light which I have found in this part of the Nebula are

1st. An irregular opening, with its centre in the position $\Delta\alpha + 108''$, $\Delta\delta + 50''$, and,

2d. A narrow channel having its axis nearly in the parallel and a declination of $\Delta\delta = +72''$ at the right ascension $\Delta\alpha = +160''$.*

It is perhaps an error to have left the light in this quarter so faint that it is liable to be overlooked, yet most of it is probably already too bright compared with other parts of the Nebula.

By the phrase "squareness of outline," I understand to be meant that the outlines have too much the appearance of straight lines, with abrupt angles at their intersections: in this sense I think there is room for improvement. At the same time any change made here for the better would affect the character and gradation of the light rather than the position of the outlines, which is the more important element.

I propose now to examine, with reference to some of the more prominent features concerned, the statement which follows the above, viz. —

"The drawing of Sir J. Herschel appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws; the only marked deviation being that the wisp of luminous matter, 10^s following and 300 divisions north in Sir John Herschel's drawing, appeared to point more towards the trapezium than there represented."

The positions given will be such as I have determined myself, unless it is otherwise stated.

* A positive sign to $\Delta\alpha$ and $\Delta\delta$ is used here, as elsewhere, to denote that the object follows θ *Orionis*, and is north of it.

Beginning with the southern shore of the *Sinus Magnus*, near the right ascension of the bright star H. 101, we have a first well-defined point of departure in the nebulous mass forming the following angle of the *Regio Huygeniana*, extending as far as the position

$$\Delta\alpha = + 145'' \qquad \Delta\delta = - 20''.$$

Of this abrupt terminus to the bright light immediately surrounding the trapezium there is no trace in Herschel's drawing. Nevertheless it is sufficiently conspicuous to have attracted the notice of Liapounov, although observing the Nebula at Kazan, under circumstances decidedly unfavourable. He has given for the position of this point (B), determined with the micrometer,

$$\Delta\alpha = + 146''{\cdot}5 \qquad \Delta\delta = - 22''{\cdot}0.$$

Herschel makes the breadth of the bright light here still 40″ to 50″, and continues the curve of the south preceding side of the Huygenian region for a distance of 180″ beyond its actual limit.

The direction of the southern shore of the *Sinus Magnus* varies considerably according to the gradation of light which is taken for its limit. The margin of bright light has a strong inclination to the parallel, to the amount of about 15° in the direction n. p. and s. f., which, however, would be variously estimated by different observers. Lamont and Liapounov have followed this margin. Herschel has taken the direction of the fainter nebulosity, which lies nearly in the parallel. In general, he has carried the bright light 10″ or 15″ too far north up to the vicinity of the trapezium.

Of the four prominences jutting out from the southern shore, forming intermediate bays, I have been able to recognise but one, that connecting with the "Bridge" (*Pons Schrœteri*), in the position

$$\Delta\alpha = + 84'' \qquad \Delta\delta = + 10''.$$

Much of the western shore is sharply defined, especially towards its northern extremity, where it is bounded by a brilliant mass referred to by many observers. This extends southward to the star H. 75, the latter being deeply immersed in the nebulosity, not on its edge, as represented in Herschel's engraving, where the shore of the gulf in this vicinity has been placed in too small right ascension, giving to the general direction of the western shore too large an inclination with the meridian, which the coast nearly follows.

The shore abruptly terminated along the bright mass referred to, passes through a point in the position

$$\Delta\alpha = + 30'' \qquad \Delta\delta = + 70''.$$

W. C. Bond, Lamont, and Liapounov, have respectively for the right ascension,

$$\Delta\alpha = +28''{\cdot}7, \quad +31'', \quad \text{and} \quad +30'',$$

each resting upon micrometric observations. Herschel's figure places this point in the right ascension

$$\Delta\alpha = +18'';$$

consequently $12''$ too far west.

The "Bridge" connects with the northern shore precisely in the position given by Herschel. This feature should extend completely across the gulf to its southern shore. Near the middle it has a conspicuous bright spot in the position

$$\Delta\alpha = +70'' \qquad \Delta\delta = +44''.$$

Liapounov has

$$\Delta\alpha = +71''{\cdot}9 \qquad \Delta\delta = +42''{\cdot}3;$$

Lamont,

$$\Delta\alpha = +80'' \qquad \Delta\delta = +52'';$$

Herschel,

$$\Delta\alpha = +72'' \qquad \Delta\delta = +61''.$$

The remainder of the northern shore is very inadequately represented in Herschel's figure, both as to the locality of the leading features, such as the "*Nebula minima*," and the terminal "Cape" at the entrance of the gulf, as well as in the direction of the outlines.

From the meridian of the star H. 87 eastward, the margin of the *Sinus Magnus* exhibits, in the engraving, a depression which brings the shore as far north as the position

$$\Delta\alpha = +110'' \qquad \Delta\delta = +120'',$$

while its true limit is in the declination $\Delta\delta = +80''$, a deviation of $40''$. A similar error of from $30''$ to $40''$ in declination affects all the neighbouring region, including the "*Nebula minima*" and the terminal "Cape." Farther north the limit of bright light is carried nearly $60''$ from its proper locality.

The point which I have called the terminal "Cape" juts out boldly upon a very dark expanse, forming, with a telescope of sufficient illuminating power, a well-defined mark for micrometric measurements in declination. Its position is

$$\Delta\alpha = +150'' \qquad \Delta\delta = +83''.$$

Liapounov has

$$\Delta\alpha = +143''{\cdot}6 \qquad \Delta\delta + 71''{\cdot}5;$$

Herschel

$$\Delta\alpha = + 152'' \qquad \Delta\delta = + 116''.$$

Lastly, the coast-line in its vicinity, and the "*Nebula minima*," have a direction inclined by less than 5° to the parallel. The angle in the engraving amounts to 20°.

We find, then, as the result of the foregoing survey, the following instances of discrepancy between Herschel's delineation of the region in question and the actual appearance of the Nebula:—

1st. The absence of a definite limit to the bright light of the Huygenian Region on its eastern side, close to the southern shore of the *Sinus magnus*, and its consequent extension along this shore to a distance of 180″ beyond its proper terminus.

2d. The bright light on the southern shore is carried 10″ to 15″ too far north.

3d. Of the four prominences on this shore only one can be certainly recognised in the positions given.

4th. In its best-defined part the western shore is placed 12″ too far to the west.

5th. The bright spot in the *Pons Schrœteri* is 15″ to 20″ too far north, and the bridge itself terminates without crossing the gulf.

6th. All the features of the northern shore to the east of the *Pons Schrœteri* have been represented in positions 30″ or 40″ north of their true locality, at the same time that the direction of the principal lines is largely in error.

To comprehend the real significance of the discrepancies here indicated, the limited extent of the area in which they occur must be borne in mind. The distance between the opposite shores of the *Sinus magnus* is but about 80″, and its length 130″. Deviations of even less than 10″ are therefore far from being insensible or unimportant. They assuredly do not warrant the opinion expressed by Messrs. Stone and Carpenter, that the engraving "appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws."

I have felt justified in entering at such length upon the evidence in the case, both on account of the prominence given to the subject by its insertion in the Annual Report of the Society, and because it is really an important one in view of the questions which have been raised respecting the reality of changes in the aspect of the Nebula. It is only by attention to the more minute details of its features, especially as regards their locality, that these questions can be decided.

Observatory of Harvard College,
May, 1864.

Discussion upon Prof. Bond's Paper.

In the discussion which followed the reading of this paper, Mr. Stone remarked that the Astronomer Royal had been desirous last year of having a new drawing of the Nebula executed, in consequence, amongst other reasons, of a wish to that effect expressed by Mr. Otto Struve, who was under the impression that considerable changes had taken place in the Nebula. That drawing was entrusted to Mr. Carpenter, and he (Mr. Stone), while it was progressing, had a great number of opportunities of observing the Nebula with the large equatoreal. It was his impression, and also Mr. Carpenter's, that there was a squareness of drawing about the "jaws," and an absence of luminosity within the jaws, in Mr. Bond's drawing, which was not seen in the Nebula itself. He mentioned this in the Report, because if a change was actually taking place, it could only be verified by the comparison of a number of drawings taken at periods separated by considerable intervals, and it was essential, in order that the conclusions drawn might be satisfactory, that the drawings adopted as standards of reference for the different epochs, should not only be correct, but should be acknowledged to be so. If errors crept in and were allowed to remain, astronomers would, a few years hence, be subjected to great inconvenience, and would probably be misled.

From the acknowledged ability of Mr. Bond, his drawing will probably become the standard of reference for the present epoch, and he (Mr. Stone) thought it most important that all suspected deviations of that drawing from accuracy should be called attention to, in order that they might be settled now, whilst an opportunity of doing so was afforded us.

In reply to Mr. Hodgson, Mr. Stone remarked that he wished to raise the point about the reality of changes inferred from the comparison of drawings, and also that he believed that Liapounov's drawing did not lay claim to any great accuracy in the drawing of the details, but was chiefly valuable on account of the great care which had been taken in laying down the relative positions of the stars and a few salient points.

Dr. Winnecke confirmed Mr. Stone's statement with respect to Liapounov's drawing, and also stated that his impression in respect to the relative accuracy of Bond's and Herschel's drawings of the parts of the Nebula about the jaws, was identical with Mr. Stone's.

Mr. Stone remarked that it must not be taken for granted that Mr. Bond's engraving fairly did his drawings justice, bearing in mind the very great difficulty of representing faithfully in any engraving the actual appearance of celestial objects—Nebulæ especially.

The Rev. C. Pritchard, who fully concurred in this remark, stated that the Society had some exquisite drawings by Mr.

Lassell, but they were quite at a loss to know by what means to get them copied.

The benefit to be derived by calling in the aid of photography was then discussed, Mr. Vignoles strongly urging the desirability of such a step.

The Rev. C. Pritchard said he doubted whether the delicate shading of the Nebulæ could be brought out by photography.

Mr. Joseph Beck bore out Mr. Vignoles' statement as to the extreme accuracy of photographic copying.

Professor Selwyn brought before the notice of the Society 144 discs of the Sun, taken between Feb. 7, 1863, and Feb. 7, 1864, by the Ely Helioautograph. In connection with this subject he referred to a recent number of the *Astronomische Nachrichten*, which contains an account of Schwabe's observation of the spots for 1863. That indefatigable observer had numbered 124 groups in 330 observations. In the diagrams which were now before the Society, there were several interesting series to which he would call especial attention, extending from Jan. 24th to Feb. 1st (being an almost uninterrupted series of 8 days), from Feb. 11th to Feb. 18th, and from April 13th to April 24th. Professor Selwyn also exhibited to the Society some autographs of the Sun on a larger scale ($6\frac{5}{8}$ in. in diameter) made in accordance with the wish expressed by Mr. De La Rue and Mr. Hodgson. The production of these pictures was a work of great difficulty, and requires considerable perseverance, especially in getting the large telescope into working order. These drawings were taken on the 16th, 17th, 18th, and 19th May, and were provided with the lines of position, the necessity for which the Astronomer Royal had always so strictly insisted upon. That was the first series he had been able to take on this scale. The next was on the 23d May, and the last (and he considered it the best of all) was taken on the 1st of June, on which day a very magnificent spot appeared. The faculæ about this spot were clearly marked in the autograph, showing, he thought, that the chemical focus of the instrument had been very nearly found.

Professor Selwyn had not had time to deduce any conclusions from these observations, but he had no doubt they would be available as illustrations of the physical phenomena of the breaking out and closing up of the spots, quite independently of those more delicate questions which had been discussed by Mr. De La Rue and Mr. Carrington (whose recently published book was a perfect treasure of knowledge on the subject.) He then referred to two observations of Mr. Balfour Stewart's (who had the Kew instrument under his charge), which he thought would receive either confirmation or contradiction from such

autographs as those now before the Society. In examining the sun-pictures taken with the Kew Heliograph under the superintendence of Mr. De la Rue, Mr. Stewart has found that it appears to be a nearly universal law that the faculæ belonging to a spot appear to the left of that spot, the motion due to the Sun's rotation being across the picture from left to right.*

As far as the speaker had observed — but he was not in a position to speak positively on the subject — this result was not supported by the Ely autographs. He believed the faculæ surrounded the spots in the same way as the edges of a crater surround the cavity of a crater, not favouring one side or the other, but lying fairly round them. The other observation made by Mr. Stewart referred to the similar behaviour of all spots on approaching the same point of elliptic longitude, which was supposed to point to planetary influence.

Prof. Selwyn felt convinced that a continuous series of autographs could not fail to give the means of proving or disproving such statements. An analogy had been drawn between the solar spots and the cyclones which occurred upon the earth's surface. There were some interesting observations on the whirling of spots made by Padre Secchi, in the Memoirs of the Roman College Observatory. In his drawings there were clear indications of a resemblance between this whirling action of the spots and the cyclones or whirling storms of the earth. Sir J. Herschel had been the first to suggest this analogy, and he (Prof. Selwyn) would press upon all observers of the Sun these two questions. First, whether the whirling of the spots was in an opposite direction in the two opposite hemispheres, *i.e.*, *contrary* to the hands of a watch in the *northern* hemisphere, and with the hands of a watch in the *southern*, which had been observed to be the law of storms on the earth; and, secondly, whether there was a difference of temperature between the equatorial and the polar regions of the Sun. Padre Secchi, by means of observations with the thermo-electric pile, had arrived at the conclusion that there was an appreciable difference in temperature between the polar and equatorial regions.

* The following table expresses the result obtained: —

Year.	No. of cases of facula to left of spot.	No. of cases of facula to right of spot.	No. of cases of facula equally on both sides of spot.	No. of cases of facula mostly between two spots.
1858	2	0	0	0
1859	18	0	0	3
1861	9	1	3	0
1862	64	4	7	3
1863	47	0	9	2
1864	18	1	2	1

Reader, April 2, 1864, page 434.

The Rev. Mr. Howlett produced for the inspection of the members a portion of a series of spots on a very enlarged scale, embodying nine successive days' observations of one spot. He had observed, in connexion with this spot on the 25th January, certain phenomena, which, although on a minute scale, might be conceived of very great importance, as affording a link in a solution of the nature of sun-spots. He began to make a drawing of a spot at 11 o'clock, and observed at that time a tolerably condensed patch of umbral tint on the extremity of the penumbra. He drew it exactly on the margin. But by the time he had finished the spot, and others on the disc, upon looking over his work for the purpose of detecting errors, he found that the patch was no longer on the margin. Believing it to be a blunder, he altered it, but in an hour or so he observed that it was conspicuously removed from the edge of the penumbra. He watched it for another hour, when his impressions were put beyond a doubt, and he called in a friend to corroborate the observation. Both himself and his friend saw that the patch was distinctly drawing rapidly towards the central umbra, and by three o'clock in the afternoon it had advanced two-thirds of the distance $= 12''$ towards the great central umbra, at the same time becoming more condensed and elongated in the direction of the usual striations upon the penumbra.

Mr. De La Rue exhibited to the Society a diagonal eye-piece, constructed on the principle recommended by Sir J. Herschel, the second surface being at right angles to the pencil of rays falling on it, by which means the second reflexion is prevented. Mr. De La Rue also explained the adjustments necessary for its use with his large reflector. He congratulated the Society on the zeal which Prof. Selwyn had displayed in following out his solar photographs. He felt that the work now accomplished at Ely was extremely valuable.

Solar Eclipses observed in China from B.C. 481 to the Christian Era. By Mr. John Williams, Assistant Secretary.

I beg leave to lay before the Society the following list of Solar Eclipses observed in China from B.C. 481 to the Christian Era. They are recorded in a Chinese historical work, entitled *Tung Keen Kang Muh*, in 101 vols. sm. 8vo. This work contains a summary of Chinese history, from the earliest times to the end of the Yuen dynasty, A.D. 1368, and was first published during the Ming dynasty, about 1473. The edition

in my possession was published in 1808. The text is exceedingly brief, consisting merely of an account of the accessions and deaths of the emperors, and of the rulers of the minor states, with some of the more remarkable occurrences in each reign. There is, however, a commentary which supplies many of the deficiencies in the text. The appointments and deaths of other eminent personages are also noticed, together with the various great calamities that have occurred at different times, such as earthquakes, inundations, storms, &c. &c. To these must be added records of Eclipses and Comets, in the order in which they occurred. The present list of Eclipses commences where those recorded in the *Chun Tsew* cease. These are also given in this work, but are not included in the present list, having formed the subject of a former communication to the Society.

I must observe that the accounts are exceedingly brief, but little more than the year, season, and moon being given, and in several instances the year only : the day is in no case noticed. They are contained in the volumes of the work from 7 to 18 inclusive.

Some of the eclipses appear to have been total, but the word expressing totality used in the *Chun Tsew* occurs but once in this list. Another phrase is used which is rather ambiguous, but, from the context, in which it is stated that the stars were visible, it can refer only to a total eclipse. In two instances a second eclipse is said to have occurred in the month immediately succeeding the first; the words, literally rendered, being in both cases, "Winter, 10th month, Sun eclipsed; 11th month, Sun eclipsed."

The dates have been carefully verified by a comparison with a set of Chronological Tables, published about 1860 in Japan. These supply not only the Chinese Cycles and their years arranged according to their *Kea Tsze* system, but also the years in the European system answering to their Cyclical years. These Tables have been of very great assistance to me, as they can be fully depended upon, and, consequently, a great amount of time and computation has been saved.

I may also add that there appears to be a continuous series of Eclipses throughout the work. I have already traced nearly 100 additional ones from the commencement of the Christian Era to about the fourth century; and I purpose, from time to time, to lay before the Society additional lists, so as to give as complete a catalogue of Solar Eclipses observed in China as the means at my disposal will allow.

It is also remarkable that hitherto I have met with no records of Lunar Eclipses. Those of Comets are numerous, and there is also an account of a conjunction of the *Woo Sing*, or five planets in or near the Lunar division *Shih* (determined by α and β *Pegasi*), which appears to have taken place between 2400 and 2500 B.C. These may form the subjects of future communications.

Solar Eclipses recorded in the Tung Keen Kang Muh.

No.	Cycle.	Year of Cycle.	Season.	Moon.	Year B.C.	
1	XXXVI.	57	Summer	5	481	
2	XXXVII.	35	o	o	443	Total; stars seen.
3	,,	43	Summer	6	435	
4	XXXVIII.	8	o	o	410	
5	,,	21	o	o	397	
6	,,	36	o	o	382	Total; stars seen.
7	,,	43	o	o	375	
8	,,	49	o	o	369	
9	XXXIX.	57	o	o	301	Total.
10	XL.	50	o	o	248	
11	XLI.	34	Winter	10	204	
12	,,	,,	,,	11	204	
13	,,	40	Summer	6	198	
14	,,	50	Spring	1	188	
15	,,	,,	Summer	5	188	Total.
16	,,	52	Summer	6	186	
17	,,	57	Spring	1	181	Total.
18	,,	60	Winter	11	178	
19	XLII.	1	Winter	10	177	
20	,,	,,	,,	11	177	
21	,,	18	Summer	4	160	
22	,,	24	Spring	1	154	
23	,,	25	Winter	10	153	
24	,,	28	Winter	11	150	
25	,,	30	Autumn	9	148	
26	,,	31	Autumn	9	147	
27	,,	32	Winter	10	146	
28	,,	34	Autumn	7	144	
29	,,	35	Autumn	7	143	
30	,,	39	Spring	2	139	
31	,,	40	Autumn	9	138	
32	,.	44	Autumn	7	134	
33	,,	51	Spring	3	127	
34	,,	56	Summer	5	122	
35	XLIII.	6	Summer	4	112	
36	,,	25	Winter	10	93	
37	,,	29	Autumn	8	89	
38	,,	34	Winter	11	84	
39	,,	49	Winter	12	69	
40	XLIV.	1	Winter	12	57	
41	,,	4	Summer	4	54	
42	,,	16	Spring	3	42	

No.	Cycle.	Year of Cycle.	Season.	Moon.	Year B.C.
43	XLIV.	18	Summer	6	40
44	,,	24	Summer	6	34
45	,,	28	Winter	12	30
46	,,	30	Summer	4	28
47	,,	32	Autumn	8	26
48	,,	33	Spring	3	25
49	,,	34	Spring	2	24
50	,,	42	Autumn	9	16
51	,,	43	Spring	2	15
52	,,	44	Spring	1	14
53	,,	45	Autumn	7	13
54	,,	46	Spring	1	12
55	,,	56	Spring	1	2
56	,,	57	Summer	4	1

On the bright band bordering the Moon's Limb in Photographs of Eclipses. By G. B. Airy, Astronomer Royal.

In a communication to the Society, printed in their *Monthly Notices*, I pointed out, as the result of ordinary optical investigation, that no refraction through a homogeneous atmosphere surrounding the Moon could account for the appearance of brightness round the Moon's dark limb. This investigation includes the case of heterogeneous atmosphere; inasmuch as a heterogeneous atmosphere may be represented to any degree of accuracy by a succession of homogeneous strata with curved dividing surfaces, and the optical conditions are not altered by separating each stratum from the next by an extremely small quantity; and then the investigation above-mentioned is to be understood as to be repeated for every successive stratum: the view after the first stratum having been proved to be, in its character of brightness, exactly the same as if there had been no first stratum, it will follow that the view after the second stratum is the same as if there had been no first and no second stratum; and so on to the end. The phenomenon in question, therefore, cannot be explained by mere optical considerations.

Professor Stephen Alexander has, with great kindness, sent me specimens of the photographs taken by him in the Eclipse of 1860. In giving my account of the appearances of these, I must premise that I am furnished with numerous pairs of spectacles carefully adapted to very distinct vision at different distances, and that I have used the appropriate spectacles, or the unarmed right eye (whose refraction is very accurate), as circumstances required.

Upon first view of the photographs, the bright band is seen, rather conspicuously; and somewhat more conspicuous, as Professor Alexander remarked, in the neighbourhood of the cusps.

Upon viewing them at different distances, the spatial breadth of the bright band varies. When most distant from the eye, the band appears broadest (in linear measure). On bringing the photograph suddenly near (the proper alteration being made in the armature of the eye), the band seems to vanish; but, on looking more carefully, it is found to exist, but much narrower than before, and thus with intermediate degrees of breadth at intermediate distances. I could not pretend to estimate accurately its angular breadth, but, roughly speaking, it seemed to me to occupy nearly the same visual angle at all times.

I cut off a strip of the thin black photographic paper by a very clean line, and cemented it to the photograph very evenly, lying right across the middle of the bright lune. The other boundary-line of the strip of paper was very slightly ragged. The bright bands were seen on both edges of the strip, but rather sharper and narrower along the clean-cut edge than along the rougher edge.

These experiments leave no doubt on my mind that the phenomenon in question is a mere nervous irritation of the retina, produced by the view of the conterminous black and white portions of the photograph.

1864, *June* 9.

Shooting Stars in March. By A. S. Herschel, Esq.

In the year 1825 Brandes, in a work describing comparative observations of shooting-stars made at Breslau in 1823, remarked that shooting-stars in general proceed from that part of the heavens towards which the Earth is tending at the time. The place of this point upon the ecliptic, called by Herrick the tangential region of the Earth — perhaps better, the Apex of the Earth's Way* — is approximately 90 degrees of longitude behind the Sun. After comparing its average position with the course of 37 shooting-stars, determined in height and direction by himself, Brandes observes, "As far as a moderate number of observations can convince me, the planetary motion of the Earth upon its orbit occasions a prevailing shower of shooting-stars in the opposite direction."†

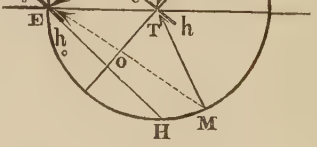
Herrick also observed in 1837 that throughout the month

* I am indebted for this term to the Rev. C. Pritchard, Sec. R. A. S.

† *Observations of Shooting Stars.* Leipzig, 1825. (p. 60.)

of November the shooting-stars at Newhaven (U. S.) pursued a prevailing direction from N.E. towards S.W. (*Silliman's Journal*, vol. xxxiii. p. 361). Continuing his observations through the winter, he added the remark that a most abundant season of meteors occurred on every night towards 6 o'clock A.M., and a season of least frequency at 6 P.M. Both of these effects he ascribed to the premises first stated by Brandes.

M. Coulvier Gravier wrote in 1845 (*Comptes Rendus*), "But for the cause which transfers, from the West to the East, nearly one-half of the number due to each of these directions, there would come exactly the same number of shooting-stars from the four points of the compass." The "cause" here intimated by M. Coulvier Gravier is the same as that formerly advanced by Brandes and by Herrick. It is ably elucidated by Mr. Bompas in the volume of the *British Association Reports for* 1858 (p. 144).



Suppose a small plane surface *e h* (fig.) to represent at once the horizon of an observer and the horizon of his Antipodes, upon the Earth, to the extent in which shooting-stars are visible to a single observer. The diameter of the circular plane *e h* will not in general exceed 200 miles (Herrick, *sup. cit.* p. 362). Let its centre T remain at rest for one second upon the trajectory of the Earth E T, at the point T: T Z perpendicular to *e h*.

If, for the purpose of calculation, shooting-stars are assumed to be cosmical particles, whose orbits intersect the orbit of the Earth at all possible angles of excentricity and inclination, *without any preference*, it follows that in general an equal number of meteors will fall upon the upper and lower surfaces of the plane *e h*, in the course of a single second.

Let the number be N, and suppose shooting-stars (on an average) to move with the velocity of the Earth in its orbit. An hour before their arrival at the plane *e h* the 2 N meteors will be found uniformly distributed upon the surface of a sphere E M H, of which the radius is 68,000 miles, the distance travelled by the Earth in an hour.

While the 2 N meteors approach towards the centre of the sphere, let the plane *e h* be carried by the Earth, from E to T along the trajectory way E T, so that when the small plane *e h* arrives at T the whole of the 2 N meteors may fall upon its two surfaces. There will fall more meteors than N upon the foremost surface of the plane *e h*, and less meteors than N upon the hindermost surface of the plane *e h*, *on account of the motion of translation of the plane.*

To know how many more meteors than N will fall upon the foremost surface of the plane eh in motion, let P represent the number, N representing, as before, the number of meteors which fall upon the same surface of the plane at rest. The proportion of P to N, or of N to P, may then be found by the following construction:—

E the Earth, and M any one of the 2 N meteors, one hour before their arrival simultaneously at T; $e_\circ h_\circ$ a small plane (equal and parallel to eh), having its centre at E. Produce the plane $e_\circ h_\circ$ until it intersects the surface of the sphere E M H in the small circle E H. The larger segment of the sphere is upon the foremost side of $e_\circ h_\circ$. Because the meteor M approaches E (in motion) along the chord M E, the whole of the meteors upon the segment E M H will at the end of the hour fall upon the foremost side of the moving plane eh.

But on the supposition that the orbits of the 2 N meteors intersect the orbit of the Earth at all possible angles of excentricity and inclination—*without preference*,—the number of meteors on the segment of the sphere, E M H, is directly proportional to the area of the segment. Therefore the number P bears the same proportion to the number N which the area of the surface of the spherical segment E M H bears to the area of a hemisphere.

P : N : : area of segment E M H : area of hemisphere
: : Z O : Z T
: : versed-sine Z T E : 1

$$\frac{P}{N} = \text{versed-sine of Nadir-distance of the apex of Earth's way} \qquad (1)$$

Having observed the number P, which is the number of meteors per second when the apex of the Earth's way has a known angular distance from the observer's Antipodes, and having divided this number by the versed-sine of the angular distance in question, the number N obtained as the quotient of this division is the number of meteors which would have fallen upon the observer's horizon, or upon the same small plane of view *per second, had the Earth remained at rest.*

Similarly (disregarding the change of Nadir-distance of the apex of the Earth's way), if W is the value of that angle at the middle of an hour when P meteors have been observed, the number *per hour, for a stationary horizon*, becomes

$$N = \frac{P}{1 - \cos W} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If shooting-stars (on an average) move with $v \times$ the velocity of the Earth in its orbit, and W is the Nadir-distance of

the apex of the Earth's way, the correction is made by the equally simple formula,

$$N = \frac{v}{v - \cos W} \times P \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

of which the former equation (2) is a corollary obtained by making $v = 1$.

There are, in all probability, 6150 independent horizons, or small planes of view, for shooting-stars, upon the whole spherical surface of the Earth, including sea and land.* If, therefore, N be the number of meteors which would fall upon any single one of these areas (could the Earth be held at rest in its place for one hour), $6150 \times N$ would represent the whole number falling in the same time upon the globe at rest. Suppose this number to be M, then,

$$M = 6150 \times \frac{v}{v - \cos W} \times P \quad . \quad . \quad . \quad . \quad (4)$$

The number M, calculated from the observed number P, the known angle W, and a hypothetical value of v is a correct measure of the scarcity or abundance of meteors at different points of the Earth's orbit. With the exception of certain extraordinary dates, it is probable that M is sensibly constant throughout the year. Writing $\frac{M}{6150} = 1$, equation (4) becomes

$$P = 1 - \frac{\cos W}{v},$$

or transforming the equation

$$P = 1 + \frac{\sin l \sin \Delta + \cos l \cos \Delta \cos H}{v} \quad . \quad . \quad (5)$$

in which l is the North latitude of the place of observation, H and Δ the hour-angle and North declination of the apex of the Earth's way.

From this expression it is seen that, M being constant, a daily *minimum* of meteoric frequency is attained at 6 P.M. (when $H = 180°$), and a daily *maximum* of meteoric frequency at 6 A.M. (when $H = 0$).

At midnight, when $H = 90°$ (approximately),

$$P = 1 + \frac{\sin l \sin \Delta}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and it is seen that, M being constant, the frequency of meteors

* Herrick, *American Journal of Science*, vol. xxxiii. p. 362.

at midnight ranges through the year (*in northern latitudes*) from

$$P_0 = 1 - \frac{\sin 23^\circ 28'}{v} \cdot \sin l \text{ (\textit{in March}) to}$$

$$P_{,} = 1 + \frac{\sin 23^\circ 28'}{v} \cdot \sin l \text{ (\textit{in September})} \quad . \quad . \quad (7)$$

If, for example, $v = 1$, the extremes of frequency in *northern latitudes from March to September* are expressed by the formula

$$P = 1 \mp \sin 23^\circ 28' \cdot \sin l \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and the extreme range of frequency

$$= 2 \sin 23^\circ 28' \cdot \sin l \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In the latitude of London, $l = 51^\circ 32'$,

In March	$P_0 = 0{\cdot}688$
In September	$P_{,} = 1{\cdot}312$

and these numbers are to one another in the proportion of 1 : 1·894.

It is remarkable that at Düsseldorf, of which the latitude differs only 19′ from that of London, this proportion of the extremes was observed by Benzenberg in 1837. Examining a single quarter of the sky on 26 ordinary nights, Benzenberg observed 1·9 shooting-stars per hour to be the average midnight frequency in spring; and 3·6 shooting-stars per hour to be the average midnight frequency in autumn.* These numbers are to one another in the proportion of 1 : 1·906, agreeing closely with the calculated ratio.

It is seen from (8) that, M being constant, the range of meteoric frequency throughout the year at midnight diminishes with the latitude. It is perhaps on this account that no seasons of meteoric scarcity are noticed by eminent travellers in tropical countries. Humboldt expresses himself as follows: "It appeared to me that meteors were more abundant than with us. This was not improbably, however, a deception which would disappear before a strict enumeration." Meteors appear to be equally frequent throughout the year in the low latitudes of the peninsula of India, and a season of scarcity is less perceptible even in the United States of America than

* Benzenberg, *Falling Stars*. Hamburgh, 1839, pp. 201–3.

in Europe, apparently on account of the smaller distance of these continents from the equator.

Not only the ordinary kinds of shooting-stars, but fireballs and aerolites, and the extraordinary displays of shooting-stars called star-showers, are more frequent in autumn than in spring. In Mr. Greg's Catalogue of 1800 Fireballs and Aerolites, the division of the year into equal portions (from January to June, and from July to December), is found to separate the occurrences of large meteors into a season of scarcity followed by a season of abundance, in the proportion of 739 to 1059 meteors, or of 1 : 1·433.* In this Catalogue the latitudes of Europe predominate among the localities. Seasons of scarcity and abundance appear with the same division of the year in M. Quetelet's Catalogue of Star-Showers (*Physique du Globe*, Brussels, 1861, p. 312), in the proportion of 28 to 72, or of 1 to 2·571. It should not be forgotten that the long and the short nights of the year are equally divided between the seasons in question, both in this instance and in the case of fireballs.

The account hitherto given of the two seasons of meteoric frequency is briefly the following : — "The Earth encounters more meteors in going from aphelion to perihelion, than in going from perihelion to aphelion."†

If the cause has been rightly traced (after the principles of Brandes, Herrick, and others) to the planetary nature of the Earth, the obliquity of the ecliptic, and to the cosmical theory of shooting-stars, this concise description of the late celebrated French astronomer Arago will require to be inverted in the Southern Hemisphere.

Shooting-stars not unfrequently cross the circle of the Sun's longitude from west to east; but their course is then protracted, and their light is red and dull, as might be expected from their diminished speed with relation to the Earth. Such shooting-stars are seen at all times of the year, but most frequently in *March*, when the apex of the Earth's way is depressed below the horizon. They serve, in the absence of Southern observations, as further confirmations of the foregoing principles of Brandes and Herrick.

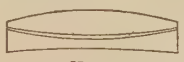
March 31, 1864.

* *British Association Report for* 1860, p. 116.

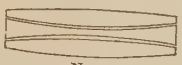
† Arago, *Popular Astronomy* (transl.). London, 1858. Vol. ii. pp. 495, 500, 505, 509.

The Achromatic Object-Glass. By R. Hodgson, Esq.

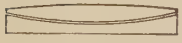
With the desire of exciting some further examination into the Achromatic Object-Glass, I have ventured to draw the attention of the Society to the various forms used by different opticians to obtain the same result, and while the form adopted by English opticians is the same as that used by the inventor John Dollond, the foreign opticians have made wide and large variations, as shown in the diagrams.



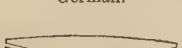
No. 1.
John Dollond, 1758.



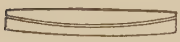
No. 2.
Peter Dollond, 1765.



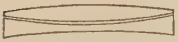
No. 3.
German.



No. 4.
Frauenhofer.



No. 5.
Sir John Herschel, 1821.



No. 6.
Professor Barlow, 1827.

No. 7.
Steinheil, 1860.

Nos. 1 and 2, the forms of the inventors John and Peter Dollond, followed by Tulley, and their successors, in double and triple object-glasses; then the German forms, Nos. 3 and 4, as applied by Frauenhofer; and now used by the French and German opticians, for the largest telescopes. The form of Sir John Herschel, No. 5, very nearly the same as Frauenhofer. Professor Barlow, No. 6, and the last, No. 7, is that of a new object-glass by Steinheil of Munich, computed from a paper by Gauss, published as long since as 1817; and from some remarks by Mr. G. P. Bond, the American astronomer, it would appear that this form admits of an increased aperture for a given focal length or a diminished focal length, with a given aperture; the light increasing in both cases.*

The able article, "Telescope," in *Rees' Cyclopedia*, by Dr. Pearson, and the paper by Prof. Barlow, in the *Phil. Transactions*, 1827, probably form the bases of computation of all the large English object-glasses made during the last few years. In the paper by Sir John Herschel (*Phil. Trans.* 1821) are several rules and formulæ; one very similar to that of Steinheil, which he characterises thus:—

"The values of the first set are, *however*, *objectionable*, as they will be found to correspond to meniscus and concavo-convex forms of the crown and flint lenses respectively, of great curvature, and placed together."

* In the recent additions to the Library of the Society, from the Turnor fund, is a curious old folio volume, "Zahn's Optics," 2d edition, printed at Nuremburg, 1702, containing a great many diagrams, amongst them the following:—

Whether the object-glass of three convex surfaces and one concave was first used in England or Germany is a doubtful point; but, upon comparing the triple object-glass, which has the two exteriors convex, it appears an easy transition to abandon one of the crown lenses and reduce the surfaces again to four, retaining the two exteriors convex.

Mathematicians and opticians have always deplored the want of good materials for object-glasses, and, although the glass at present made cannot be called perfect, yet, in comparison with that manufactured twenty or even ten years ago, it must be admitted to be excellent, and, with the improvement in material and the application of steam power to machinery for grinding and polishing, it would be satisfactory to know that the form of the English object-glass is the best that can be used. The regularity or stability of the glass now made by Messrs. Chance of Birmingham (the variation probably being within a range of from three to five per cent) would, perhaps, admit of a few short tables being computed, to enable the working optician to make at all times a good object-glass, according to rules laid down, instead of labouring in the dark by trial and error to arrive at what too often is but an indifferent result.*

* The object-glass produced at the Meeting, of 4 inches aperture and 63 inches focal length, was made in 1849, after a careful reading of Prof. Barlow's paper. The refractive and dispersive values of the glasses were found from small lenses made of similar glass, according to the directions laid down. The curves were then calculated, and the tools made to the respective radii. The four sides were each only *ground once* and *polished once*, and the object-glass will on favourable nights show the five stars in the trapezium of *Orion*, and all other stars usually seen with good telescopes of 4 inches aperture.

On μ Herculis. By the Rev. W. R. Dawes.

I have the pleasure of announcing the curious fact, that the small star *s. p. μ Herculis*, which in the year 1856 was discovered by Mr. Alvan Clark to be close double, is a *binary* system; and that within the last five or six years the variation in position, as deduced from my own observations, amounts to about 18°. Thus the resemblance of this triple system to that of *μ Boötis* is complete. As I remarked in my notes appended to the first series of new double stars discovered by Mr. Clark, it was ascertained many years ago by Professor Struve that this small star has a common proper motion with *μ Herculis*; as is also the case with *μ Boötis* and the small double star which accompanies it.

I measured the companion of *μ Herculis* twice in the year 1857, but found it extremely difficult. In 1859 I obtained

three sets of measures more easily, and of better quality, with my present telescope. Allowing the weights assigned to the different sets, the mean comes out,

P = 59°·91; obs. 32; w. 116 : D = 2″·025; obs. 20; w. 50; epoch 1858·77.

The night of the 6th of this month being very fine, I turned my telescope upon this object, and was instantly struck with the large variation in the angle. This was made so readily apparent by the fact, that in 1859 the close stars pointed almost precisely to μ; but at present the line joining their centres makes an angle of 15° or 16° with the line joining μ and the brightest of the close pair.

The result of the single night's measures is,

P = 77°·59; obs. 5; w. 32 : D = 1″·806; obs. 6; w. 17; epoch 1864·43.

Comparing these with the mean above quoted, we find an increase of the angle amounting to 17°·68 in 5·66 years, or $3\frac{1}{8}$° in one year; while the distance has probably not varied perceptibly.

As the position and distance of the smaller star with respect to μ were observed by Struve at Dorpat on six nights in three different years, and at Poulkowa so lately as 1851, without the duplicity of the companion being perceived, it seems almost necessary to conclude that the distance must have been very much less at that time. Yet considerable doubt is thrown upon this by the fact that, if my observations may be relied on, no increase in the distance has taken place since 1857.

On the same night (June 6) I obtained good measures of *Antares*, which gave

P = 175°·71; obs. 5; w. 28 : D = 3″·675; obs. 10; w. 30; epoch 1864·43.

The small star was frequently well seen, and quite free from the light round its brilliant neighbour. There seems at present to be no certain proof of any change having taken place in this star; but the observations, on the whole, point to a slight increase of both the angle and the distance.

Hopefield Observatory, Haddenham, Bucks,
1864, *June* 8.

On the large Sun-spot Period of about 56 *Years.*

By Balfour Stewart, M.A. F.R.S.

Besides the well-known period of 11·2 years, Prof. Wolf has found, both fron sun-spot records and from those of Auroræ, a longer period of about 56 years, which had its maximum in 1836.

From the recent comparison instituted by Mr. Carrington we have grounds for supposing that a connexion may, perhaps, exist between the radius vector of *Jupiter* and the development of Sun-spots, such that, when *Jupiter* is furthest from the Sun, there are most spots. If this be true, it is reasonable to suppose that, if at any epoch both *Jupiter* and *Saturn* came to aphelion together, we ought then to have had a remarkable development of spots. No doubt both of these planets are far from the Sun; but, on the other hand, they are of great mass and of considerable excentricity.

Let us suppose that we had such an epoch now, since two revolutions of *Saturn* are very nearly equal to five of *Jupiter*, 59 years afterwards we should have the same planetary phenomenon repeated, and also another remarkable development of Sun-spots. So far this accounts very well for Prof. Wolf's period of 56 years.

Let us now examine the dates of aphelion of these two planets, and find at what epoch, according to our hypothesis, we ought to have had a maximum of spot-frequency.

In the following table these dates for both planets are given and compared together:—

Date of Aphelion of Jupiter.	Date of Aphelion of Saturn.	Difference.
1815·4	1812·1	+3·3
1839·2	1841·6	−2·4
1874·9	1871·1	+3·8

We thus see that the dates of aphelion of the two planets come closest together about 1840, which is not far distant from 1836, the date of maximum given by Prof. Wolf.

New Minor Planet. By N. R. Pogson, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

"I think the identity of my last new planet with *Freia* is now quite established, and the name *Sappho* again at liberty for future use. It was one which Sir J. Herschel had suggested to me as suitable for a small planet, and hence my preference for it. I have, therefore, much pleasure in providing another candidate, the first being disqualified; but this one most certainly and evidently new. It was first seen on May 2 and mapped as a star, but its planetary nature was not suspected until the following night.

"Its partially reduced positions then, and its approximate last recorded place, are

	Madras M.T.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
May 3	13 44 11	16 12 3·4	106 47 11
	14 44 4	16 12 0·9	106 46 55
12	$12\frac{1}{2}$	16 4 9	105 46 0

Magnitude 10·4 Daily Motions — 53^{s}, + $6\frac{3}{5}'$.

"*Freia* was observed on twenty-five nights: this one, so far, on four.

"*Madras*, 1864, *May* 14."

Mr. De La Rue reports most favourably of the performance of a telescope of new construction sent by Dr. Steinheil for exhibition at his reception held at Willis's Rooms on June 11th. The objective has an aperture of 48 lines (about $4\frac{2}{10}$ inches English) and a focal length of only 40 inches (French). It is not the objective of Gauss, already made public. Mr. W. T. Radford has purchased the telescope, so that it will remain in England. Dr. Steinheil is now engaged upon an object-glass composed of three lenses of 6 inches aperture, and only 30 inches focal length.

From a communication received from M. Le Verrier, it appears that his meaning with respect to relative value of eye-and-ear and touch observations, has been in some measure misapprehended. M. Le Verrier's only intention, in the conversation reported in the May Number of the *Monthly Notices*, p. 159, was to express that, in his opinion, a touch-observer does not acquire the same appreciation of duration of time as an observer trained to eye-and-ear observations.

Although the discussion at the Meeting appears to have been correctly reported, the Council regret that they have given publicity to a citation of an oral conversation, which conveys to some extent an erroneous statement of the views of the illustrious Director of the Imperial Observatory of Paris.

ERRATUM.

In Monthly Notices, No. 7.

Page 165, line 12, *after* the Rev. W. R. Dawes, *insert* that their existence is is at least doubtful.

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